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Biogeosciences Discussions

Interactive comment on "Mass, nutrients and oxygen budgets for the North Eastern Atlantic Ocean" *by* G. Maze et al.

Anonymous Referee #1

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This manuscript seeks to optimally estimate the transports of mass and several tracers (nitrate, phosphate and oxygen) in a Northeast Atlantic region (a box bounded roughly by Iceland, Greenland and Iberian Peninsula) using several years (2002, 2004, 2006) of currents and tracer observations and historical data and transport estimates. The optimization is carried out by minimizing the residuals of volume and tracer budget within the box at the same time. In a sense, this is assuming the temporal variability of the mass and tracer inventory within the studied domain are small, as compared to transport or other terms (air-sea fluxes, biological sources and sinks etc). The overall conclusions are that 1) a majority (70%) of transport by NE Atlantic Current crosses the Reykjanes Ridge and subsequently returns southeastward, 2) the study region is a carbon sink and 3) as such autotrophic. Moreover, the air-sea oxygen flux is dominated by the abiotic processes (thermal and mixing).

I think the overall approach is reasonable and should be able to yield more robust estimates of the budgets than previous studies. The current conclusions seem consistent with current understanding in the studied area. However, there are several questionable aspects in the method implementation (detailed below), which renders the estimates questionable. Therefore, I recommend a significant revision of the manuscript.

Key issues,

1) The quasi-steady state assumption. While I believe the inter-annual changes of water mass and tracer inventory in this area are small compared to transport divergence, air-sea fluxes and biological sinks. However, it is not obvious how small these terms are. Some justifications of this assumption will be needed, which maybe in a form of inter-annual variability of, for example, NO3 along the OVIDE transect, and perhaps some rough estimates. If there is strong inter-annual variability of NO3, then minimization of residuals is a wrong approach.

2) I am not sure why oxygen solubility should be included in the optimization process. Solubility itself depends on temperature, salinity and pressure, and therefore it is not a conserved parameter. Therefore equation (4), if it should be included, should have a source/sink term on the right-hand-side, in addition to transport and air-sea fluxes. I would recommend removing this from the optimization.

3) Authors claim on bottom of page 4328 that only the transport is optimized. This appears to contradict the Table 1, where biological and air-sea exchange terms also noted as optimized. Throughout the main text, though, it appears that indeed only transport is optimized and air-sea exchanges and biological terms are merely derived from mass balance in (2)-(6). However, if this is the case, (2) and (3) will create obvious inconsistency as along the boundary N/P may not exactly follow Redfield ratio. Therefore, some clarification will be needed.

4) The determination of mean tracer concentrations along the box boundaries (equation B2). Such an approach is problematic given a) transport as claimed are less known

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and their estimates are likely most unreliable, and b) there are clearly two-layer current structures in some area, which will amplify the uncertainty of tracer concentration estimate. I believe a straightforward volumetric average of observed concentrations should be a better approach.

There are also some minor issues. For example, horizontal mixing and air-sea freshwater fluxes are neglected. One might argue that either horizontal mixing is small or it can be treated as transport. For the former, a justification will be helpful. For the latter, mixing term depends on gradient of tracers, which is mathematically different from the advetional divergence. As a separate term, it can also be optimized. Similarly, omission of E-P needs to be justified.

It seems that the priori estimates of biological source/sink of NO3 have little value because the final values are much smaller. One should note that Lee (2001)'s estimates of NCP are values for the euphotic zone, whereas in this paper the biological source/sink represent the net values for the entire water column, and naturally, are much smaller given much more remineralization occur below the euphotic zone.

In addition, so-called biotic air-sea flux (page 4335 lines 13-17, Fig 4) seems to me is a misleading name. Nutrients or oxygen requirements by biological removal or production can be realized through either horizontal transport or air-sea fluxes. So it cannot be totally assigned to air-sea exchange (i.e., not an air-sea flux term).

Some other minor issues,

Page 4327, line 12, "Here after" should be "Hereafter". Page 4330, line 21, "bar if" should be "bar is". Page 4333, line 5, "organic matter" should be "phytoplankton" Page 4340, line 16, "found" should be "find"

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